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Beside experimenting with insects, a series of experiments was conducted with the hope of ascertaining what fruits are preferred by catbirds. Equal volumes of cherries and mulberries were placed on the cork island, and at other times the experiment was repeated with strawberries in place of the cherries, and invariably the mulberries were selected. When there were any hopes of getting mulberries, the birds never touched strawberries or cherries. In the next experiment red and white mulberries were both put into the cage; the birds always took the red ones first. This last experiment showed that catbirds can distinguish colors. The whole series of experiments showed mulberries are preferred to cherries or strawberries, hence it may be inferred that these two latter crops can be protected from catbirds by planting mulberries. This preference for mulberries could not be deduced from stomach examination alone, for the reason that mulberries are not common in many places where catbird stomachs were collected. Experiments with other fruits would have been performed, if my birds had not been killed by a cat.

In recapitulation I would say that in investigating the food of a bird, the first thing to be done is to examine enough stomachs to obtain a general idea of the bird's food. After this has been done, one can intelligently go into the field and watch birds feeding. The different kinds of available food should be noted before collecting stomachs, then it will be possible to ascertain what the bird will eat, its preferences, and what it will refuse.

DR. ALEX. GOETTE ON THE DEVELOPMENT OF THE VERTEBRAL COLUMN.

By O. P. Hay.

In "Zeitschrift für wissenschaftliche Zoologie," Vol. LXII, pp. 343–394, Dr. Alex. Goette has published a paper entitled "Ueber den Wirbelbau bei den Reptilien und einigen anderen

Wirbelthieren." In this paper, besides detailing the results of his studies on certain lizards, the author considers views held or supposed to be held by Dr. E. D. Cope, Dr. G. Baur, and myself concerning the morphogeny of the vertebral column. In the present paper I shall endeavor to vindicate the position I have taken on the subject. Drs. Cope and Baur are capable of making their own defense.

My conclusions regarding the mode of development of the vertebral column were reached after a careful study of the young of Amia and a comparison of the results with the vertebral structures of other animals, living and extinct. These conclusions were set forth in "Publications of Field Columbian Museum," Vol. I, pp. 1–54; and it is to this paper that my distinguished critic refers. That paper really consists of two parts; the first part dealing with the structures of the adult axial skeleton, the second, beginning with page 25, treating of the development of the vertebral column in the young fish. The views expressed in the first part are somewhat modified in the second.

It may be well first of all to correct some errors into which Dr. Goette has fallen regarding statements made by myself. On page 381 of his paper he affirms that I found in the embryo of Amia, between the bases of the arches and the notochord, a dense layer of connective tissue, which later disappeared. I really found nothing of the kind, and I know of no expression in my paper which suggests it. A statement somewhat to this effect is, however, made by Dr. Gadow and Miss Abbott in "Phil. Trans. Roy. Soc. London," Vol. 186, p. 202; but there is no indication given that Goette had seen this publication.

On the same page of Goette's paper occurs the statement that I discovered that in dorsal region of Amia the upper intercalated cartilages push themselves under the succeeding dorsal arches, lift the latter away from contact with the notochord, and then fuse with them. Goette also refers to figure 10 of my paper as representing such a condition. The assertion indicates a complete misconception of both the text and the figure. What I said was that *at a very early period* these intercalated cartilages may have been fused with the arches. After they have once become distinct they never again fuse with the arches, and my figure represents them as being entirely distinct.

It is also to be noted that I disclaim holding the view that the intervertebral menisci of the Amniota have anything to do with the degenerated hypocentra.

A considerable portion of Goette's essay is devoted to a defense of a publication by Dr. Ludwig Schmidt, on which I made some remarks. I am unable to find anything in my former paper to the effect that Amia calva, being the most recent fish of the group, cannot possibly retain the embolomerous structure in case this were the older. It requires only a cursory perusal of that paper to discover that, after a study of the young of Amia, I did not regard the embolomerous condition as more recent than the rhachitomous. I need here only to call attention to page 41 of that paper.

Furthermore, I leave it to unprejudiced readers to decide whether or not I did Dr. Schmidt injustice when I affirmed that he had given two irreconcilable explanations of the way in which the simple vertebræ of the dorsal region had resulted from the embolomerous condition of the tail. Schmidt's first effort is plainly directed toward showing that the dorsal vertebræ have originated through the direct union of two such disks as occur in the middle of the tail. He describes and figures an abnormal vertebra which had been produced by the fusion of a " centrum " and an " intercentrum." He figures a section of this vertebra and calls attention to the " rudimentary arch " of the " centrum " and to the double-cone-shaped cavity between the two disks. Then, referring to the dorsal vertebræ, he notes their close resemblance to the united disks of the tail, the presence of the rudimentary arch, and then endeavors to explain the absence of the notochordal cavity on the ground of the very early union of the elements. This is a procedure wholly without point, in case one of the elements has become wholly or almost wholly reduced. Dr. Baur had already suggested this very natural explanation, and the only fault that Schmidt found with Baur's idea was that the latter regarded the "intercentrum" as the enlarged base of the lower arches, instead of a complete vertebral body.

From the consideration of the vertebræ of Amia from this point of view Schmidt turns to the discussion of its fossil relatives; and here he gives a different solution of the problem, the one that Goette endeavors to have us accept as the only one proposed by his pupil. The "intercentrum" is regarded as increasing at the expense of the "centrum," until in Megalurus and Amia the latter element is reduced to a mere vestige, the "rudimentary upper arch." The first method of union is that of two nearly equal elements, the latter the union of the lion and the lamb—with the lamb inside of the lion.

So far from being open to the charge of opposing the view that the dorsal vertebræ are constituted of elements homologous with those found in the somites of the tail, with reduction of some of them and expansion of others, that is the very pith of the embryological portion of my former paper.

I may remark here that in Amia the pleurocentral element can hardly be regarded as vestigial, since it appears to furnish the foundation for the upper half of the vertebral centrum. Not merely the small portion of the cartilage which is seen in front of the upper arch, but the whole of the cartilage in the upper half of the dorsal centrum, belongs to the pleurocentrum. The figures of Callopterus reproduced by Goette are very interesting, inasmuch as they show to what extent in that form the above element had become reduced. When the reduction becomes complete, the vertebral centrum becomes a hypocentrum, a condition affirmed by Cope to be that of the higher fishes in general.

Aside from any misunderstandings, there exist between Goette and myself certain differences which are fundamental. He holds that there is to be found in most vertebrates a specialized sheath which, composed of cells, surrounds the notochord and becomes segmented to form the "primary vertebral centra." This sheath, called in other publications "äussere oder zellige Chordascheide," is now named the "perichordal sheath." In some of Goette's writings it appears to be certain that he has in mind the elastica externa of other writers; in other papers it is evident that he refers to a somewhat specialized layer of cells belonging to the skeletogenous tissue. According to Goette, the arches, upper and lower, are formed in the skeletogenous sheath outside of this perichordal sheath, and only later become applied to it. The primary vertebra thus consists of the arches, upper and lower, and the ring around the notochord. Furthermore, Goette contends that, primitively at least, two such vertebræ belong to each somite. In few or no animals do we find both vertebræ present in all their parts, but the author referred to finds vestiges of them in even the highest vertebrates. I cannot subscribe to these views.

Without desiring to detract in the least from the merits of Goette's embryological labors, I believe that I am not wrong in saying that the importance, even the existence, of this perichordal sheath has not been recognized by vertebrate embryologists. Most writers deny that the alleged sheath is anything more than a portion of the general skeletogenous layer and hold that it graduates into the latter. Hasse appears to come nearer than others in recognizing a special layer of the skeletogenous tissue; but his "innere Zellschicht" does not appear to correspond wholly with Goette's perichordal sheath, since the former invests also the spinal cord. Furthermore, Hasse finds his layer of cells in Acipenser, to which genus Goette has denied the perichordal sheath.

It is very apparent too that in one great group of vertebrates, the Elasmobranchs, Goette's theory of the origin of the primary vertebral body has been demonstrated to be erroneous. Instead of there being, in these fishes, an "äussere Chordascheide" which has arisen as a distinctly specialized layer of the cells of the skeletogenous tissue, and which is a little later cut off from the less modified portions of this tissue by the elastica externa, it has been demonstrated by Klaatsch and Gadow and Abbott that the layer of cells called by Goette "äussere Chordascheide", takes its origin from migrating cells which, starting from the bases of the arches, have pierced the elastica externa and made their way into the fibrous mass of the elastica interna. The "primary vertebral centrum" of the sharks must then be a thing very different from what it is in the other groups of vertebrates.

Considering that the limits of the "äussere Chordascheide," its significance, and even its existence, are not yet agreed upon, the alleged product of its transformation, the "primary vertebral body" is a thing too intangible to be long considered in the presence of manifest realities.

Goette claims that in the tail of Amia the two disks belonging to each somite are two nearly equally developed vertebræ; it happening that only the arches of one of them are in a vestigial condition. I do not betieve that the elements which give rise to the two rings are equivalent. The arches, basidorsals and basiventrals of Gadow, arise at a later period than do the intercalated cartilages; at least they do so in Amia. The arches occupy a position essentially different from that of the intercalated elements, the former being placed intermyomerically, the latter myomerically. The arches have probably been evolved as a means for the fixed attachment for the muscular segments: the intercalated cartilages as a system of stop-gaps. Later these subordinate pieces have in many cases assumed a more important role.

I have been able to discover no reason for supposing that there is, in each somite of Amia, either one or two "primary vertebral centra." For, if by "perichordal sheath" Goette possibly refers to the elastica externa, this in Amia is certainly homologous with the structure so-called in the Teleost fishes and which, according to all recent observers, is not cellular. If by perichordal sheath Goette means a distinct layer of cells, which lies against the outside of the elastica externa and passes beneath the arches, then there is no such sheath. In my smallest specimens, 10 mm. long, a delicate layer of cells surrounds the notochord, but it does not show itself as a special layer under the bases of the arches, although the latter are not yet distinctly chondrified. Nor is there at any stage any such a well defined layer of tissue under the cartilages. Conversion of the arches into hyaline cartilage begins at a little distance away from the elastica, but when the process is completed, the cartilage comes into immediate contact with the elastica. Nor is there any modified layer of cartilage next to the elastica. Furthermore, when ossification begins, the layer of bone does not pass between the base of the arch and the elastica, but over the sides of the arch and against the elastica from an upper arch to one below it and to its fellow piece.

For the original duplication of vertebræ Goette finds evidences in the Urodeles, lizards, etc., in what he regards as the occurrence of vestigial upper arches, transverse processes, and ribs. These, it seems to me, are as yet of too uncertain nature for us to base on them any such serious conclusions as does Dr. Goette. They are probably susceptible of being otherwise homologised. Goette has reproached the palæontologists for deriving their theories from their palæontological, rather than from embryological, studies. But, are the embryological materials any more to be relied upon to furnish safe conclusions than are the materials used by the palæontologist? Under the strata of what countless generations the primitive structures have been buried! How many elements that once were prominent and perhaps all-important have been totally suppressed, or if vestiges of them remain in the embryo, how difficult it is to detect and to interpret them! How many adaptive modifications have been introduced into perhaps every species! Among the embryologists themselves there are many who declare that ontogony offers little reliable evidence regarding phylogeny. This I am not ready to admit. The palæontologist must not despise the embryologist; nor must the latter scorn the former. We shall do well if we succeed in explaining nature after we have made use of all her aids.

As regards the duplication of ribs, a doctrine of Goette's, one set for each supposed vertebral centrum, one pair must, so far as I see, have fallen in the intermuscular septum, the other in the middle of the myomere. The latter is a condition unknown, hardly conceivable. Two ribs placed at different levels in the same intermuscular septum might belong easily to the same vertebral body, as in the case of many fishes. How fortunate it is that the shad has not inherited a full complement of vertebræ and their appendages!

Goette objects to the views entertained by Cope, Baur and myself, since they lead to the conclusion that the vertebræ are not homologous throughout the various groups of vertebrates. They do not need to be homologous and are not so. The vertebral centra of the sharks, arising as they do from the invasion of cells into the elastica interna, cannot be homologous with those of the Teleostomes, which originate in a skeletogenous tissue outside of the elastica externa. An abnormal vertebra of the tail of Amia formed by coalescence of the two disks of a somite, is not homologous with one of the disks, even if we were, with Goette, willing to regard the latter as vertebræ. Nor is a simple vertebra of the dorsal region, made up as it is of parts of two alleged vertebræ, homologous with anything that we find in the tail.

Of course, Goette holds that the " primary vertebral centrum" is found in all the Digitata, and is developed wholly independent of the arches. So far as the Amphibia are concerned, I believe that the centrum is primitively derived from the arches, even in the fossil Branchiosauridæ. We do not need to suppose that cartilage has, in all cases, surrounded the notochord where bone is now found. As in Amia, the ossification may spread from the bases of the arches into the soft connective tissue lying against the notochord. It may even be so precocious in its appearance as to suppress the cartilaginous stage of the arches, as is the case with many fishes.

I find that in the young Amia a thin layer of cartilage is formed under the bone of the centrum, lying close against the elastica. It appears to spread from the bases of the arches, and is developed later than the bone. Possibly the ancestors of Amia possessed a more exstensively developed condition of this cartilage.

A comparison of the early condition of the vertebral column of the Urodeles with that of Lepisosteus brought me to the conclusion that the intervertebral cartilages of both are homologous with the " intercalated cartilages " of Amia, and Dr. Gadow in a recent publication has adopted the same view. Without attempting to explain all of Hasse's and Field's results, I believe that they are entirely in error when they affirm that the earliest rudiment of the vertebral centrum is a segment of the elastica externa, and that the cells of the skeletogenous tissue which develop into the intervertebral cartilages first make their way through the elastica. Whatever that earliest rudiment may be, it has nothing to do with the elastica externa. This is present, as it is in fishes, in close relation with the elastica interna. In the tail of young Necturus, where the bone is already well developed, the externa may be plainly seen as a highly refractive line between the interna and the centrum. But this is nothing new, since other observers have seen the externa distinct from the centrum.

As regards the lizards and other Amniota, I am willing to concede that there intervenes between the bases of the arches and the sheath of the notochord a distinct cartilage on which rest the bases of the upper arches. Goette's " primary vertebral centrum " found in the lizards I regard as the pleurocentrum, which has been pushed under the bases of the arches. If it is such, we might expect to find it starting in its development on the upper side of the notochord ; and Goette's figure shows at least that it is thinner on the lower side than elsewhere. I am ready to admit that in the Amniota the basalia, to use Gadow's terms, have formed unions with the interbasals behind them, instead of with those in front of them. The possibility of this was considered in my former paper, p. 51. Consequently, in the vertebra figured by Goette Figs. 1, 2, 7, the centrum has been pushed forward under the arch in front of it. It is quite possible that its original myomeral position is no longer reproduced in the embryo.

If our minds can once be freed from the idea of a primary centrum we shall probably find little reason for disagreement about the development of the vertebræ in the different groups. It seems to me beyond doubt that the rhachitomous vertebræ of the dorsal region of Eurycormus (Zittel, Handbuch, vol. 3, p. 230) must have been developed from the embolomerous vertebræ of the tail. We have seen how the rhachitomous vertebræ of the dorsal region of the young Amia unite to produce the definitive vertebræ. Thiollière's figures of Callopterus, reproduced by Goette, show us how, by the reduction of the pleurocentrum, the vertebral body becomes a hypocentrum. If now the pleurocentrum should grow at the expense of all the other elements, we would have such a vertebra as Cope and others find in the Amniota. And considering what we find in the temnospondylous Stegocephali, in the Clepsydropidæ, and in Sphenodon, I cannot refuse to believe that such a course of development has been pursued.

It appears to me unnecessary to suppose that the embolomerous condition has at any time resulted from the rhachitomous. In some cases probably the latter has grown out of the former; but it is by no means a necessary course. Probably in most cases the lower intercalated cartilages have suffered reduction before coösification has united them with the upper intercalated pieces.

THE SEVENTH SESSION OF THE INTERNATIONAL GEOLOGICAL CONGRESS.

This will probably eclipse any previous session in many respects and will enjoy the distinction hitherto conceded to the second, or that of Bologna, of being the most notable concourse of geologists, and of making the most important contribution to geological knowledge, both practical and theoretical, in the history of this Congress. Many circumstances conspire to produce this result. In the first place Russians share with Americans the reputation of being lavish in expenditure and prodigal in hospitality; it may be added of both nations also, that this extravagance is not diminished because it furthers some important project.

When at the London session of 1888, it was decided to hold the fifth session of the Congress in Philadelphia in 1891, the geological world was prepared to be astounded at the profusion of the hospitality and the generosity exhibited in the general management and especially in the long excursions. That the bacillus of Officialism infected this egg before it was laid in the wrong nest, and broke hope's promise as to its hatching, only